The apparent orbit is:

Major axis = $2''\cdot656$
Minor axis = $1''\cdot480$
Angle of major axis = $173^\circ\cdot5$
,, ,, periastron = $186^\circ\cdot7$
Distance of star from centre = $0''\cdot638$

The computed and observed places seem to justify the new elements given above. The period thus will hardly be varied by as much as ten years, while the resulting alteration will be small in proportion.

THE BRITISH ASSOCIATION.

SECTION K.

BOTANY.

OPENING ADDRESS BY W. T. THISELTON-DYER, M.A., F.R.S., C.M.G., C.I.E., DIRECTOR OF THE ROYAL GARDENS.

THE establishment of a new Section of the British Association, devoted to Botany, cannot but be regarded by the botanists of this country as an event of the greatest importance. For it is practically the first time that they have possessed an independent organisation of their own. It is true that for some years past we have generally been strong enough to form a separate department of the old Biological Section D, on the platform of which so many of us in the past have acted in some capacity or other, and on which indeed many of us may be said to have made our first appearance. We shall not start then on our new career without the remembrance of filial affection for our parent, and the earnest hope that our work may be worthy of its great traditions.

The first meeting of the Section, or, as it was then called, Committee, at Oxford was held in 1832. And though there has been from time to time some difference in the grouping of the several biological sciences, the two great branches of biology have only now for the first time formally severed the partnership into which they entered on that occasion. That this severance, if inevitable from force of circumstances, is in some respects a matter of regret, I do not deny. Specialisation is inseparable from scientific progress; but it will defeat its own end in biology if the specialist does not constantly keep in touch with those fundamental principles which are common to all organic nature. We shall have to take care that we do not drift into a position of isolation. Section D undoubtedly afforded a convenient opportunity for discussing many questions on which it was of great advantage that workers in the two different fields should compare their results and views. But I hope that by means of occasional conferences we shall still, in some measure, be able to preserve this advantage.

RETROSPECT.

I confess I found it a great temptation to review, however imperfectly, the history and fortunes of our subject while it belonged to Section D. But to have done so would have been practically to have written the history of botany in this country since the first third of the century. Yet I cannot pass over some few striking events.

I think that the earliest of these must undoubtedly be regarded as the most epoch-making. I mean the formal publication by the Linnean Society, in 1833, of the first description of "the nucleus of the cell," by Robert Brown ("Misc. Bot. Works," i. 512). It seems difficult to realise that this may be within the recollection of some who are now living amongst us. It is, however, of peculiar interest to me that the first person who actually distinguished this all-important body, and indicated it in a figure, was Francis Bauer, thirty years earlier, in 1802. This remarkable man, whose skill in applying the resources of art to the illustration of plant anatomy has never, I suppose been surpassed, was "resident draughtsman for fifty years to the Royal Botanic Garden at Kew." And it was at Kew, and in a tropical orchid, *Phaius grandifolius*, no doubt grown there, that the discovery was made.

It was, I confess, with no little admiration that, on refreshing my memory by a reference to Robert Brown's paper, I read again the vivid account which he gives in a footnote of the phenomena, so painfully familiar to many of us who have been teachers, exhibited in the staminal hair of *Tradescantia*. Sir Joseph Hooker (*Proc. Linn. Soc.*, 1887-88, 65) has well remarked that "the supreme importance of this observation, . . . leading to undreamt-of conceptions of the fundamental phenomena of organic life, is acknowledged by all investigators." It is singular that so profound an observer as Robert Brown should have himself missed the significance of what he saw. The world had to wait for the discovery of protoplasm by Von Mohl till 1846, and till 1850 for its identification with the sarcode of zoologists by Cohn, who is still, I am happy to say, living and at work, and to whom last year the Linnean Society did itself the honour of presenting its medal.

The Edinburgh meeting of the Association, in 1834, was the occasion of the announcement of another memorable discovery of Robert Brown's. I will content myself with quoting Hofmeister's ("Higher Cryptogamia," 432) account of it. "Robert Brown was the discoverer of the polyembryony of the *Coniferæ*. In a later treatise he pointed out the origin of the pro-embryo in large cells of the endosperm, to which he gave the name of corpuscula." The period of the forties, just half a century ago, looks in the retrospect as one of almost dazzling discovery. To say nothing of the formal appearance of protoplasm on the scene, the foundations were being laid in all directions of our modern botanical morphology. Yet its contemporaries viewed it with a very philosophical calm. Thwaites, who regarded Carpenter as his master, described at the Oxford meeting in 1847 the conjugation of the *Diatomaceæ*, and "distinctly indicated," as Carpenter ("Memorial Sketch," 140) says, "that conjugation is the primitive phase of sexual reproduction." Berkeley informed me that the announcement fell perfectly flat. A year or two later Suminski came to London with his splendid discovery (1848) of the archegonia of the fern, the antheridia having been first seen by Nägeli in 1844. Carpenter (*loc. cit.*, 141) gave me, many years after, a curious account of its reception. "At the Council of the Ray Society, at which," he said, "I advocated the reproduction of Suminski's book on the 'Ferns,' I was assured that the close resemblance of the antherozoids to spermatozoa was quite sufficient proof that they could have nothing to do with vegetable reproduction. I do not think," he added—and the complaint is pathetic—"that the men of the present generation, who have been brought up in the *light*, quite apprehend (in this as in other matters) the utter darkness in which we were then groping, or fully recognise the deserts of those who helped them to what they now enjoy." This was in 1875, and I suppose is not likely to be less true now.

The Oxford meeting in 1860 was the scene of the memorable debate on the origin of species, at which it is interesting to remember that Henslow presided. On that occasion Section D reached its meridian. The battle was Homeric. However little to the taste of its author, the launching of his great theory was, at any rate, dignified with a not inconsiderable explosion. It may be that it is not given to the men of our day to ruffle the dull level of public placidity with disturbing and far-reaching ideas. But if it were, I doubt whether we have, or need now, the fierce energy which inspired then either the attack or the defence. When we met again in Oxford last year the champion of the old conflict stood in the place of honour, acclaimed of all men, a beautiful and venerable figure. We did not know then that that was to be his farewell.

The battle was not in vain. Six years afterwards, at Nottingham, Sir Joseph Hooker delivered his classical lecture on Insular Floras. It implicitly accepted the new doctrine, and applied it with admirable effect to a field which had long waited for an illuminating principle. The lecture itself has since remained one of the corner-stones of that rational theory of the geographical distribution of plants which may, I think, be claimed fairly as of purely English origin.

HENSLOW.

Addressing you as I do at Ipswich, there is one name written in the annals of our old Section which I cannot pass over—that of Henslow. He was the Secretary of the Biological Section at its first meeting in 1832, and its President at Bristol in 1836. I suppose there are few men of this century who have indirectly more influenced the current of human thought. For in great measure I think it will not be contested that we owe Darwin to him. As Romanes has told us ("Memorial Notices," 13): "His letters written to Prof. Henslow during his voyage round the world overflow with feelings of affection, veneration, and obligation to his accomplished master and dearest friend—feelings which throughout his life he retained with no diminished intensity. As he used himself to say, before he knew Prof.

Henslow the only objects he cared for were foxes and partridges." I do not wish to overstate the facts. The possession of "the collector's instinct, strong in Darwin from his childhood, as is usually the case in great naturalists," to use Huxley's (*Proc. R.S.*, xliv. vi.) words, would have borne its usual fruit in after life, in some shape or other, even if Darwin had not fallen into Henslow's hands. But then the particular train of events which culminated in the great work of his life would never have been started. It appeared to me, then, that it would not be an altogether uninteresting investigation to ascertain something about Henslow himself. The result has been to provide me with several texts, which I think it may be not unprofitable to dwell upon on the present occasion.

In the first place, what was the secret of his influence over Darwin? "My dear old master in Natural History" ("Life," ii. 317) he calls him; and to have stood in this relation to Darwin[1] is no small matter. Again, he speaks of his friendship with him as "a circumstance which influenced my whole career more than any other" (i. 52). The singular beauty of Henslow's character, to which Darwin himself bore noble testimony, would count for something, but it would not in itself be a sufficient explanation. Nor was it that intellectual fascination which often binds pupils to the master's feet; for, as Darwin tells us, "I do not suppose that any one would say that he possessed much original genius" (i. 52). The real attraction seems to me to be found in Henslow's possession, in an extraordinary degree, of what may be called the Natural History spirit. This resolves itself into keen observation and a lively interest in the facts observed. "His strongest taste was to draw conclusions from long-continued minute observations" (i. 52). The old Natural History method, of which it seems to me that Henslow was so striking an embodiment, is now, and I think unhappily, almost a thing of the past. The modern university student of botany puts his elders to blush by his minute knowledge of some small point in vegetable histology. But he can tell you little of the contents of a country hedgerow; and if you put an unfamiliar plant in his hands he is pretty much at a loss how to set about recognising its affinities. Disdaining the field of nature spread at his feet in his own country, he either seeks salvation in a German laboratory or hurries off to the Tropics, convinced that he will at once immortalise himself. But *cœlum non animum mutat;* he puts into "pickle" the same objects as his predecessors, never to be looked at again; or perhaps writes a paper on some obvious phenomena which he could have studied with less fatigue in the Palm House at Kew.

The secret of the right use of travel is the possession of the Natural History instinct, and to those who contemplate it I can only recommend a careful study of Darwin's "Naturalist's Voyage." Nothing that came in his way seems to have evaded him or to have seemed too inconsiderable for attention. No doubt some respectable travellers have lost themselves in a maze of observations that have led to nothing. But the example of Darwin, and I might add of Wallace, of Huxley, and of Moseley, show that that result is the fault of the man and not of the method. The right moment comes when the fruitful opportunity arrives to him who can seize it. The first strain of the prelude with which the "Origin" commences are these words: "When on board H.M.S. *Beagle* as naturalist, I was much struck with certain facts in the distribution of the organic beings inhabiting South America." But this sort of vein is not struck at hazard or by him who has not served a tolerably long apprenticeship to the work.

When one reads and re-reads the "Voyage," it is simply amazing to see how much could be achieved with a previous training which we now should think ludicrously inadequate. Before Henslow's time the state of the natural sciences at Cambridge was incredible. In fact, Leonard Jenyns ("Memoir," 175), his biographer, speaks of the "utter disregard paid to Natural History in the University previous to his taking up his residence there." The Professor of Botany had delivered no lectures for thirty years, and though Sir James Smith, the founder of the Linnean Society, had offered his services, they were declined on the ground of his being a Nonconformist (*ibid.*, 37).

As to Henslow's own scientific work, I can but rely on the judgment of whose who could appreciate it in relation to its time. According to Berkeley (*ibid.*, 56), "he was certainly one of the first, if not the very first, to see that two forms of fruit might exist in the same fungus." And this, as we now know, was a fundamental advance in this branch of morphology. Sir Joseph Hooker tells me that his papers were all distinctly in advance of his day. Before occupying the chair of botany, he held for some years that of mineralogy. Probably he owed this to his paper on the Isle of Anglesey, published when he was only twenty-six. I learn from the same authority, that this to some extent anticipated, but at any rate strongly influenced, Sedgwick's subsequent work in the same region.

BOTANICAL TEACHING.

Henslow's method of teaching deserves study. Darwin says of his lectures "that he liked them much for their extreme clearness." "But," he adds, "I did not study botany" (i. 48). Yet we must not take this too seriously. Darwin ("Voyage," 421), when at the Galapagos, "indiscriminately collected every thing in flower on the different islands, and fortunately kept my collections separate." Fortunately indeed; for it was the results extracted from these collections, when worked up subsequently by Sir Joseph Hooker, which determined the main work of his life. "It was such cases as that of the Galapagos Archipelago which chiefly led me to study the origin of species" (iii. 159).

Henslow's actual method of teaching went some way to anticipate the practical methods of which we are all so proud. "He was the first to introduce into the botanical examination for degrees in London the system of practical examination" ("Memoir," 161). But there was a direct simplicity about his class arrangements characteristic of the man. "A large number of specimens . . . were placed in baskets on a side-table in the lecture-room, with a number of wooden plates and other requisites for dissecting them after a rough fashion, each student providing himself with what he wanted before taking his seat" (*ibid.*, 39). I do not doubt that the results were, in their way, as efficient as we obtain now in more stately laboratories.

The most interesting feature about his teaching was not, however, its academic aspect, but the use he made of botany as a general educational instrument. "He always held that a man of *no* powers of observation was quite an exception" (*ibid.*, 163). He thought (and I think he proved) that botany might be used "for strengthening the observant faculties and expanding the reasoning powers of children in all classes of society" (*ibid.*, 99). The difficulty with which those who undertake now to teach our subject have to deal is that most people ask the question, What is the use of learning botany unless one means to be a botanist? It might indeed be replied that as the vast majority of people never learn anything effectively, they might as well try botany as anything else. But Henslow looked only to the mental discipline; and it was characteristic of the man and of his belief in his methods that when he was summoned to Court to lecture to the Royal family, his lectures "were, in all respects, identical with those he was in the habit of giving to his little Hitcham scholars" ("Memoir," 149); and it must be added that they were not less successful.

This success naturally attracted attention. Botanical teaching in schools was taken up by the Government, and continues to receive support to the present day. But the primitive spirit has, I am afraid, evaporated. The measurement of results by means of examination has been fatal to its survival. The teacher has to keep steadily before his eyes the necessity of earning his grant. The educational problem retires into the background. "The strengthening of the observant faculties," and the rest of the Henslowian programme must give way to the imperious necessity of presenting to the examiner candidates equipped with at least the minimum of text-book formulas reproducible on paper. I do not speak in this matter without painful experience. The most astute examiner is defeated by the still more astute crammer. The objective basis of the study on which its whole usefulness is built up is promptly thrown aside. If you supply the apple blossom for actual description, you are as likely as not to be furnished with a detailed account of a buttercup. The training of observation has gone by the board, and the exercise of mere memory has taken its place. But a table of logarithms or a Hebrew grammar would serve this purpose equally well. Yet I do not despair of Henslow's work still bearing fruit. The examination system will collapse from the sheer impossibility of carrying it on beyond a certain point. Freed from its trammels, the teacher will have greater scope for individuality, and the result of his labours will be rewarded after some intelligent system of inspection. And here I may claim support from an unexpected quarter. Mr. Gladstone has recently written to a correspondent:—"I think that the neglect of natural history, in all its multitude of branches, was the grossest defect of our old system of training for the young; and, further, that little or nothing has been done by way of remedy for that defect in the attempts made to alter or reform that system." I am sure that the importance and weight of this testimony, coming as it does from one whose training and sympathies have always been literary, cannot be denied. That there is already some revival of Henslow's methods, I judge from the fact that I have received applications from Board Schools, amounting to some hundreds, for surplus specimens from the Kew Museums. Without a special machinery for the purpose I cannot do much, and perhaps it is well. But my staff have willingly done what was possible, and from the letters I have received I gather that the labour has not been wholly misspent.

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[1] As I shall have frequent occasion to quote the "Life and Letters" I shall insert the references in the text.

MUSEUM ARRANGEMENT.

This leads me to the last branch of Henslow's scientific work on which I am able to touch, that of the arrangement of museums, especially those which being local have little meaning unless their purpose is strictly educational. I think it is now generally admitted that, both in the larger and narrower aspects of the question, his ideas, which were shared in some measure by Edward Forbes, were not merely far in advance of his time, but were essentially sound. And here I cannot help remarking that the zoologists have perhaps profited more by his teaching than the botanists. I do not know how far Sir William Flower and Prof. Lankester would admit the influence of Henslow's ideas. But, so far as my knowledge goes, I am not aware that, at any rate in Europe, there is anything to be seen in public museums comparable to the educational work accomplished by the one at the College of Surgeons and the Natural History Museum, and by the other at Oxford.

I have often thought it singular that in botany we have not kept pace in this matter with our brother naturalists. I do not doubt that vegetable morphology and a vast number of important facts in evolution, as illustrated from the vegetable kingdom, might be presented to the eye in a fascinating way in a carefully arranged museum. The most successful and, indeed, almost the only attempt which has been made in this direction is that at Cambridge, which, I believe, is due to Mr. Gardiner. But our technical methods for preserving specimens still leave much to desire. Something more satisfactory will, it may be hoped, some day be devised, and the whole subject is one which is well worth the careful consideration of our Section. Henslow at least effected a vast improvement in the mode of displaying botanical objects; and a collection prepared by his own hands, which was exhibited at one of the Paris exhibitions, excited the warm admiration of the French botanists, who always appreciate the clear illustration of morphological facts.

OLD SCHOOL OF NATURAL HISTORY.

If the old school of natural history of which Henslow in his day was a living spirit is at present, as seems to be the case, continually losing its hold upon us, this has certainly not been due to its want of value as an educational discipline, or to its sterility in contributing new ideas to human knowledge. Darwin's "Origin of Species" may certainly be regarded as its offspring, and of this Huxley (*Proc. R.S.*, xliv. xvii.) says with justice: "It is doubtful if any single book except the 'Principia,' ever worked so great and rapid a revolution in science, or made so deep an impression on the general mind." Yet Darwin's biographer, in that admirable "Life" which ranks with the few really great biographies in our language, remarks (i. 155): "In reading his books one is reminded of the older naturalists rather than of the modern school of writers. He was a naturalist in the old sense of the word, that is, a man who works at many branches of science, not merely a specialist in one." This is no doubt true, but does not exactly hit off the distinction between the kind of study which has gone out of fashion and that which has come in. The older workers in biology were occupied mainly with the external or, at any rate, grosser features of organisms and their relation to surrounding conditions; the modern, on the other hand, are engaged on the study of internal and intimate structure. Work in the laboratory, with its necessary limitations, takes the place of research in the field. One may almost, in fact, say that the use of the compound microscope divides the two classes. Asa Gray has compared Robert Brown with Darwin as the "two British naturalists" who have "more than any others, impressed their influence upon science in the nineteenth century" (NATURE, x. 80). Now it is noteworthy that Robert Brown did all his work with a simple microscope. And Francis Darwin writes of his father: "It strikes us nowadays as extraordinary that he should have had no compound microscope when he went his *Beagle* voyage; but in this he followed the advice of Robert Brown, who was an authority on such matters" (i. 145). One often meets with persons, and sometimes of no small eminence, who speak as if there were some necessary antagonism between the old and the new studies. Thus I have heard a distinguished systematist describe the microscope as a curse, and a no less distinguished morphologist speak of a herbarium having its proper place on a bonfire. To me I confess this anathematisation of the instruments of research proper to any branch of our subject is not easily intelligible. Yet in the case of Darwin himself it is certain that if his earlier work may be said to rest solely on the older methods, his later researches take their place with the work of the new school. At our last meeting Pfeffer vindicated one of his latest and most important observations.

The case of Robert Brown is even more striking. He is equally great whether we class him with the older or the modern school. In fact, so far as botany in this country is concerned, he may be regarded as the founder of the latter. It is to him that we owe the establishment of the structure of the ovule and its development into the seed. Even more important were the discoveries to which I have already referred, which ultimately led to the establishment of the group of Gymnosperms. "No more important discovery," says Sachs ("History," 142), "was ever made in the domain of comparative morphology and systematic botany. The first steps towards this result, which was clearly brought out by Hofmeister twenty-five years later, were secured by Robert Brown's researches, and he was incidentally led to these researches by some difficulties in the construction of the seed of an Australian genus." Yet it may be remembered that he began his career as naturalist to Flinders's expedition for the exploration of Australia. He returned to England with 4000 "for the most part new species of plants." And these have formed the foundation of our knowledge of the flora of that continent. Brown's chief work was done between 1820 and 1840, and, as Sachs (*loc. cit.*, 139, 140) tells us, "was better appreciated during that time in Germany than in any other country."

MODERN SCHOOL.

The real founder of the modern teaching in this country in both branches of biology I cannot doubt was Carpenter. The first edition of his admirable "Principles of Comparative Physiology" was published in 1838, the last in 1854. All who owe, as I do, a deep debt of gratitude to that book will agree with Huxley ("Memorial Sketch," 67) in regarding it as "by far the best general survey of the whole field of life and of the broad principles of biology which had been produced up to the time of its publication. Indeed," he adds, "although the fourth edition is now in many respects out of date, I do not know its equal for breadth of view, sobriety of speculation, and accuracy of detail."

The charm of a wide and philosophic survey of the different forms under which life presents itself could not but attract the attention of teachers. Rolleston elaborated a course of instruction in zoology at Oxford in which the structures described in the lecture-room were subsequently worked out in the laboratory. In 1872 Huxley organised the memorable course in elementary biology at South Kensington which has since, in its essential features, been adopted throughout the country. In the following year, during Huxley's absence abroad through ill-health, I arranged, at his request, a course of instruction on the same lines for the Vegetable Kingdom.

That the development of the new teaching was inevitable can hardly be doubted, and I for my part am not disposed to regret the share I took in it. But it was not obvious, and certainly it was not expected, that it would to so large an extent cut the ground from under the feet of the old Natural History studies. The consequences are rather serious, and I think it is worth while pointing them out.

In a vast empire like our own there is a good deal of work to be done and a good many posts to be filled, for which the old Natural History training was not merely a useful but even a necessary preparation. But at the present time the universities almost entirely fail to supply men suited to the work. They neither care to collect, nor have they the skilled aptitude for observation. Then, though this country is possessed at home of incomparable stores of accumulated material, the class of competent amateurs who were mostly trained at our universities, and who did such good service in working that material out, is fast disappearing. It may not be easy indeed in the future to fill important posts even in this country with men possessing the necessary qualifications. But there was still another source of naturalists, even more useful, which has practically dried up. It is an interesting fact that the large majority of men of the last generation who have won distinction in this field have begun their career with the study of medicine. That the kind of training that Natural History studies give is of advantage to students of medicine which, rightly regarded, is itself a Natural History study, can hardly be denied. But the exigencies of the medical curriculum have crowded them out; and this, I am afraid, must be accepted as irremediable. I cannot refrain from reading you, on this point, an extract from a letter which I have received from a distinguished official lately entrusted with an important foreign mission. I should add that he had himself been trained in the old way.

"I have had my time, and must leave to younger men the delight of working these interesting fields. Such chances never will occur again, for roads are now being made and ways cut in the jungle and forest, and you have at hand all sorts of trees level on the ground ready for study. These bring down with them orchids, ferns, and climbers of many kinds, including rattan palms, &c. But, excellent as are the officers who devote their energy to thus opening up this country, there is not one man who knows a palm from a dragon-tree, so the chance is lost. Strange to say, the medical men of the Government service know less and care less for Natural History than the military men, who at least regret they have no training or study to enable them to take an intelligent interest in what they see around them. A doctor nowadays cares for no living thing larger or more complicated than a *bacterium* or a *bacillus*."

But there are other and even more serious grounds why the present dominance of one aspect of our subject is a matter for regret. In the concluding chapter of the "Origin," Darwin wrote: "I look with confidence to the future—to young and rising naturalists." But I observe that most of the new writers on the Darwinian theory, and, oddly enough, especially when they have been trained at Cambridge, generally begin by more or less rejecting it as a theory of the origin of species, and then proceed unhesitatingly to reconstruct it. The attempt rarely seems to me successful, perhaps because the limits of the laboratory are unfavourable to the accumulation of the class of observations which are suitable for the purpose. The laboratory, in fact, has not contributed much to the Darwinian theory, except the "Law of Recapitulation," and that, I am told, is going out of fashion.

The Darwinian theory, being, as I have attempted to show, the outcome of the Natural History method, rested at every point on a copious basis of fact and observation. This more modern speculation lacks. The result is a revival of transcendentalism. Of this we have had a copious crop in this country, but it is quite put in the shade by that with which we have been supplied from America. Perhaps the most remarkable feature is the persistent vitality of Lamarckism. As Darwin remarks: "Lamarck's one suggestion as to the cause of the gradual modification of species—effort excited by change of conditions—was, on the face of it, inapplicable to the whole vegetable world" (ii. 189). And if we fall back on the inherited direct effect of change of conditions, though Darwin admits that "physical conditions have a more direct effect on plants than on animals" (ii. 319), I have never been able to convince myself that that effect is inherited. I will give one illustration. The difference in habit of even the same species of plant when grown under mountain and lowland conditions is a matter of general observation. It would be difficult to imagine a case of "acquired characters" more likely to be inherited. But this does not seem to be the case. The recent careful research of Gaston Bonnier only confirms the experience of cultivators. The modifications acquired by the plant when transported for a definite time from the plains to the Alps, or *vice versâ*, disappear at the end of the same period when the plant is restored to its original conditions (*Ann. d. Sc. nat.*, 7^e sér. xx. 355).

Darwin, in an eloquent passage, which is too long for me to quote ("Origin," 426), has shown how enormously the interest of Natural History is enhanced "when we regard every production of nature as one which has had a long history," and "when we contemplate every complex structure . . . as the summing up of many contrivances." But this can only be done, or at any rate begun, in the field, and not in the laboratory.

A more serious peril is the dying out amongst us of two branches of botanical study in which we have hitherto occupied a position of no small distinction. Apart from the staffs of our official institutions, there seems to be no one who either takes any interest in, or appreciates in the smallest degree, the importance of systematic and descriptive botany. And geographical distribution is almost in a worse plight, yet Darwin calls it, "that grand subject, that almost keystone of the laws of creation" (i. 356).

I am aware that it is far easier to point out an evil than to remedy it. The teaching of botany at the present day has reached a pitch of excellence and earnestness which it has never reached before. That it is somewhat one-sided cannot probably be remedied without a subdivision of the subject and an increase in the number of teachers. If it has a positive fault, it is that it is sometimes inclined to be too dogmatic and deductive. Like Darwin, at any rate in a biological matter, "I never feel convinced by deduction, even in the case of H. Spencer's writings" (iii. 168). The intellectual indolence of the student inclines him only too gladly to explain phenomena by referring them to "isms," instead of making them tell their own story.

Organisation of Section.

I am afraid I have detained you too long over these matters, on which I must admit I have spoken with some frankness. But I take it that one of the objects of our Section is to deliver our minds of any perilous stuff that is fermenting in it. But now, having taken leave of the past, let us turn to the future.

We start at least with a clean slate. We cannot bind our successors, it is true, at other meetings. But I cannot doubt that it will be in our power to materially shape our future, notwithstanding. When we were only a department I think we all felt the advantage of these annual meetings, of the profitable discussion, formal and informal, and of the privilege of meeting so many of our foreign brethren who have so generously supported us by their presence and sympathy.

I am anxious, then, to suggest that we should conduct our proceedings on as broad lines as possible. I do not think we should be too ready to encourage papers which may well be communicated to societies, either local or central.

The field is large; the labourers as they advance in life can hardly expect to keep pace with all that is going on in it. We must look to individual members of our number to help us by informing and stimulating addresses on subjects they have made peculiarly their own, or on important researches on which they have been specially engaged.

Nomenclature.

There is one subject upon which, from my official position elsewhere, I desire to take the opportunity of saying a few words. It is that of Nomenclature. It is not on its technical side, I am afraid, of sufficient general interest to justify my devoting to it the space which its importance would otherwise deserve. But I hope to be able to enlist your support for the broad common-sense principles on which our practice should rest.

As I suppose, every one knows we owe our present method of nomenclature in natural history to Linnæus. He devised the binominal, or, as it is often absurdly called, the binomial system. That we must have a technical system of nomenclature I suppose no one here will dispute. It is not, however, always admitted by popular writers who have not appreciated the difficulty of the matter, and who think all names should be in the vernacular. There is the obvious difficulty that the vast majority of plants do not possess any names at all, and the attempts to manufacture them in a popular shape have met with but little success. Then, from lack of discriminating power on the part of those who use them, vernacular names are often ambiguous; thus Bullrush is applied equally to *Typha* and to *Scirpus*, plants extremely different. Vernacular names, again, are only of local utility, while the Linnean system is intelligible throughout the world.

A technical name, then, for a plant or animal is a necessity, as without it we cannot fix the object of our investigations into its affinity, structure, or properties ("Linn. Phil.," 210). "Nomina si nescis perit et cognito rerum."

In order to get clear ideas on the matter let us look at the logical principles on which such names are based. It is fortunate for us that these are stated by Mill, who, besides being an authority on logic, was also an accomplished botanist. He tells us ("System of Logic," i. 132): "A naturalist, for purposes connected with his particular science, sees reason to distribute the animal or vegetable creation into certain groups rather than into any others, and he requires a name to bind, as it were, each of his groups together." He further explains that such names, whether of species, genera, or orders, are what logicians call connotative; they *denote* the members of each group, and *connote* the distinctive characters by which it is defined. A species, then, connotes the common characters of the individuals belonging to it; a genus, those of the species; an order, those of the genera.

But these are the logical principles, which are applicable to names generally. A name such as *Ranunculus repens* does not differ in any particular from a name such as John Smith, except that one denotes a species, the other an individual.

This being the case, and technical names being a necessity, they continually pass into general use in connection with horticulture, commerce, medicine, and the arts. It seems obvious that, if science is to keep in touch with human affairs, stability in nomenclature is a thing not merely to aim at, but to respect. Changes become necessary, but should never be insisted upon without grave and solid reason. In some cases they are inevitable unless the taxonomic side of botany is to remain at a standstill. From time to time the revision of a large group has to be undertaken from a uniform and comparative point of view. It then often occurs that new genera are seen to have been too hastily founded on insufficient grounds, and must therefore be merged in others. This may involve the creation of a large number of new names, the old ones becoming henceforth a burden to literature as synonyms. It is usual in such cases to retain the specific portion of the original name, if possible. If it is, however, already preoccupied in the genus to which the transference is made, a new one must be devised. Many modern systematists have, however, set up the doctrine that a specific epithet once given is indelible, and whatever the taxonomic wanderings of the organism to which it was once assigned, it must always accompany it. This, however, would not have met with much sympathy from Linnæus, who attached no importance to the specific epithet at all: "Nomen specificum sine generico est quasi pistillum sine campana" ("Phil.," 219). Linnæus always had a solid reason for everything he did or said, and it is worth while considering in this case what it was.

Before his time the practice of associating plants in genera had made some progress in the hands of Tourneford and others, but specific names were still cumbrous and practically unusable. Genera were often distinguished by a single word; and it was the great reform accomplished by Linnæus to adopt the binominal principle for species. But there is this difference. Generic names are unique, and must not be applied to more than one distinct group. Specific names might have been constituted on the same basis; the specific name in that case would then have never been used to designate more than one plant, and would have been sufficient to indicate it. We should have lost, it is true, the useful information which we get from our present practice in learning the genus to which the species belongs; but theoretically a nomenclature could have been established on the one-name principle. The thing, however, is impossible now even if it were desirable. A specific epithet like *vulgaris* may belong to hundreds of different species belonging to as many different genera, and taken alone is meaningless. A Linnean name, then, though it consists of two parts, must be treated as a whole. "Nomen omne plantarum constabit nomine generico et especifico" ("Phil," 212). A fragment can have no vitality of its own. Consequently, if superseded, it may be replaced by another which may be perfectly independent.[1]

It constantly happens that the same species is named and described by more than one writer, or different views are taken of specific differences by various writers; the species of one are therefore "lumped" by another. In such cases, where there is a choice of names, it is customary to select the earliest published. I agree, however, with the late Sereno Watson (NATURE, xlvii. 54) that "there is nothing whatever of an ethical character inherent in a name, through any priority of publication or position, which should render it morally obligatory upon any one to accept one name rather than another." And in point of fact Linnæus and the early systematists attached little importance to priority. The rigid application of the principle involves the assumption that all persons who describe or attempt to describe plants are equally competent to the task. But this is far from being the case that it is sometimes all but impossible even to guess what could possibly have been meant.[1]

In 1872 Sir Joseph Hooker ("Flora of British India," i. vii.) wrote: "The number of species described by authors who cannot determine their affinities increases annually, and I regard the naturalist who puts a described plant into its proper position in regard to its allies as rendering a greater service to science than its describer when he either puts it into a wrong place or throws it into any of those chaotic heaps, miscalled genera, with which systematic works still abound." This has always seemed to me not merely sound sense, but a scientific way of treating the matter. What we want in nomenclature is the maximum amount of stability and the minimum amount of change compatible with progress in perfecting our taxonomic system. Nomenclature is a means, not an end. There are perhaps 150,000 species of flowering plants in existence. What we want to do is to push on the task of getting them named and described in an intelligible manner, and their affinities determined as correctly as possible. We shall then have material for dealing with the larger problems which the vegetation of our globe will present when treated as a whole. To me the botanists who waste their time over priority are like boys who, when sent on an errand, spend their time in playing by the roadside. By such men even Linnæus is not to be allowed to decide his own names. To one of the most splendid ornaments of our gardens he gave the name of *Magnolia grandiflora*: this is now to be known as *Magnolia fœtida*. The reformer himself is constrained to admit, "The change is a most unfortunate one in every way" ("Garden and Forest," ii. 615). It is difficult to see what is gained by making it, except to render systematic botany ridiculous. The genus *Aspidium*, known to every fern cultivator, was founded by Swartz. It now contains some 400 species, of which the vast majority were, of course, unknown to him at the time; yet the names of all these are to be changed because Adamson founded a genus, *Dryopteris*, which seems to be the same thing as *Aspidium*. What, it may be asked, is gained by the change? To science it is certainly nothing. On the other hand, we lumber our books with a mass of synonyms, and perplex every one who takes an interest in ferns. It appears that the name of the well-known Australian genus *Banksia* really belongs to *Pimelea*; the species are therefore to be renamed, and *Banksia* is to be rechristened *Sirmuellera*, after Sir Ferdinand von Mueller; a proposal which, I need hardly say, did not emanate from an Englishman.

I will not multiply instances. But the worst of it is that those who have carefully studied the subject know that, from various causes which I cannot afford the time to discuss, when once it is attempted to disturb accepted nomenclature it is almost impossible to reach finality. Many genera only exist by virtue of their redefinition in modern times; in the form in which they were originally promulgated they have hardly any intelligible meaning at all.

It can hardly be doubted that one cause of the want of attention which systematic botany now receives is the repulsive labour of the bibliographical work with which it has been overlaid. What an enormous bulk nomenclature has already attained may be judged from the "Index Kewensis," which was prepared at Kew, and which we owe to the munificence of Mr. Darwin. In his own studies he constantly came on the track of names which he was unable to run down to their source. This the "Index" enables to be done. It is based, in fact, on a manuscript index which we compiled for our own use at Kew. But it is a mistake to suppose that it is anything more than the name signifies, or that it expresses any opinion as to the validity of the names themselves. That those who use the book must judge of for themselves. We have indexed existing names, but we have not added to the burden by making any new ones for species already described.

What synonymy has now come to may be judged by an example supplied me by my friend Mr. C. B. Clarke. For a single species of *Fimbristylis* he finds 135 published names under six genera. If we go on in this way we shall have to invent a new Linnæus, wipe out the past, and begin all over again.

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[1] As Alphonse de Candolle points out in a letter published in the *Bull. de la Soc. bot. de France* (xxxix.), "the real merit of Linnæus has been to combine, for all plants, the generic name with the specific epithet." It is important to remember that in a logical sense the "name" of a species consists, as Linnæus himself insisted, in the combination, not in the specific epithet, which is a mere fragment of the name, and meaningless when taken by itself.

[1] Darwin, who always seems to me, almost instinctively, to take the right view in matters relating to natural history, is ("Life," vol. i. p. 364) dead against the new "practice of naturalists appending for perpetuity the name of the *first* describer to species." He is equally against the priority craze:—"I cannot yet bring myself to reject very *well-known* names" (*ibid.*, p. 369).

Although I have brought the matter before the Section it is not one in which this, or indeed any collective assembly of botanists, can do very much. While I hope I shall carry your assent with the general principles I have laid down, it must be admitted that the technical details can only be appreciated by experienced specialists. All that can be hoped is a general agreement amongst the staffs of the principal institutions in different countries where systematic botany is worked at; the free-lances must be left to do as they like.

PUBLICATIONS.

I have dwelt at such length on certain aspects of my subject that perhaps, without great injustice, you may retort on me the complaint of one-sidedness But when I survey the larger field of botany in this country, the prospect seems to me so vast that I should despair even if I had my whole address at my disposal of doing it justice. I think that its extent is measured by the way in which the publications belonging to our subject are maintained. First of all we have access to the Royal Society, a privilege of which I hope we shall always continue to take advantage for communications which either treat of fundamental subjects, or at least are of general interest to biologists. Next to this we have our ancient Linnean Society, with a branch of its publications handsomely and efficiently devoted to systematic work. Then we have the *Annals of Botany*, which has now, I think, established its position, and which brings together the chief morphological and physiological work accomplished in the country. Lastly, we have the *Journal of Botany*, a less ambitious but useful periodical, which is mainly devoted to the labours of English botanists. I remember there was a time when I thought that this, at any rate, was an exhausted field. But it is not so; knowledge in its most limited aspects is inexhaustible if the labourer have the necessary insight. The discoveries of Mr. Arthur Bennett amongst the potamogetons of the Eastern Counties is a striking and brilliant instance.

Besides the publication of the *Annals* we owe to the Oxford Press a splendid series of the best foreign text-books issued in our own language. If the thought has sometimes occurred to one's mind that we were borrowers too freely from our indefatigable neighbours, I, at least, remember that the late Prof. Eichler paid us the compliment of saying that he preferred to read one of these monumental books in the English translation rather than in the original. I believe it is no secret that botany owes the aid that Oxford has rendered it in these and other matters in great measure to my old friend the Master of Pembroke College, than whom I believe science has no more devoted supporter.

PALÆOBOTANY.

I have said much of recent botany; I must not pass over that of past ages. Two notable workers in this field have passed away since our last meeting. Saporta was with us at Manchester, and we shall not readily forget his personal charm. If some of his work has about it a too imaginative character, the patience and entire sincerity with which he traced the origin of the existing forms of vegetation in Southern Europe to their ancestors in the not distant geological past will always deserve attentive study. But in the venerable, yet always useful, Williamson we lose a figure whose memory we shall long preserve. With rare instinct he accumulated a wealth of material illustrative of the vegetation of the Carboniferous epoch, which, I suppose, is unique in the world. And this was prepared for examination with incomparable patience either by his own hands or under his own eyes. He illustrated it with absolute fidelity. And if he did not in describing it always use language with which we could agree, nothing could ruffle either his imperturbable good nature or the noble simplicity of his character. Truth to tell, we were often in friendly warfare with him. But I rejoice to think that before his peaceful end came he had patiently reconsidered and abandoned all that we regarded as his heresies, but which were, in truth, only the old manner of looking at things. And I think that if anything could have contributed to make his departure happy, it was the conviction that the completion of his work and his scientific reputation would remain perfectly secure in the hands of Dr. Scott.

VEGETABLE PHYSIOLOGY.

Turning again to the present, the difficulty is to limit the choice of topics on which I would willingly dwell. In an address which I delivered at the Bath meeting in 1888, I ventured to point out the important part which the action of enzymes would be found to play in plant metabolism. My expectations have been more than realised by the admirable work of Prof. Green on the one hand, and of Mr. Horace Brown on the other. The wildest imagination could not have foreseen the developments which in the hands of animal physiologists would spring from the study of the fermentative changes produced by yeast and bacteria. These, it seems to me, bid fair to revolutionise our whole conceptions of disease. The reciprocal action of ferments, developed in so admirable a manner by Marshall Ward in the case of the ginger-beer plant, is destined, I am convinced, to an expansion scarcely less important.

But, perhaps, the most noteworthy feature in recent work is the disposition to reopen in every direction fundamental questions. And here, I think, we may take a useful lesson from the practice of the older Sections, and adopt the plan of entrusting the investigation of special problems to small committees, or to individuals who are willing to undertake the labour of reporting upon special questions which they have made peculiarly their own. These reports would be printed *in extenso*, and are capable of rendering invaluable service by making accessible acquired knowledge which could not be got at in any other way.

We owe to Mr. Blackman a masterly demonstration of the fact, long believed, but never, perhaps, properly proved, that the surface of plants is ordinarily impermeable to gases. Mr. Dixon has brought forward some new views about water-movement in plants, which I confess I found less instructive than many of my brother botanists. They are expressed in language of extreme technicality; but, as far as I understand them, they amount to this. The water moving in the plant is contained in capillary channels; as it evaporates at the surface of the leaves a tensile strain is set up, as long as the columns are not broken, to restore the original level. I can understand that in this way the "transpiration current" may be maintained. But what I want to know is how this explains the phenomena in the sugar maple, a single tree of which will yield, I believe, 20-30 gallons of fluid before a single leaf is expanded.

We owe to Messrs. Darwin and Acton the supply of a "Manual of Practical Vegetable Physiology," the want of which has long been keenly felt. Like the father of one of the authors, "I love to exalt plants" (i. 98). I have long been satisfied that the facts of vegetable physiology are capable of being widely taught, and are not less significant and infinitely more convenient than most of those which can be easily demonstrated on the animal side. How little any accurate knowledge of the subject has extended was conspicuously demonstrated in a recent discussion at the Royal Society, when two of our foremost chemists roundly denied the existence of a function of respiration in plants, because it was unknown to Liebig!

ASSIMILATION.

The greatest and most fundamental problem of all is that of assimilation. The very existence of life upon the earth ultimately depends upon it. The veil is slowly, but I think surely, being lifted from its secrets. We now know that starch, if its first visible product, is not its first result. We are pretty well agreed that this is what I have called a "proto-carbohydrate." How is the synthesis of this effected? Mr. Acton, whose untimely end we cannot but deeply deplore, made some remarkable researches, which were communicated to the Royal Society in 1889, on the extent to which plants could take advantage of organic compounds made, so to speak, ready to their hand. Loew, in a remarkable paper, which will perhaps attract less attention than it deserves from being published in Japan (*Bull. College of Agric. Imp. Univ. Tokio*, vol. i.), has from the study of the nutrition of bacteria, arrived at some general conclusions in the same direction. Bokorny appears recently to have similarly experimented on algæ. Neither writer, however, seems to have been acquainted with Acton's work. The general conclusion which I draw from Loew is to strengthen the belief that form-aldehyde is actually one of the first steps of organic synthesis, as long ago suggested by Adolph Baeyer. Plants, then, will avail themselves of ready-made organic compounds which will yield them this body. That a sugar can be constructed from it has long been known, and Bokorny has shown that this can be utilised by plants in the production of starch.

The precise mode of the formation of form-aldehyde in the process of assimilation is a matter of dispute. But it is quite clear that either the carbon dioxide or the water, which are the materials from which it is formed, must suffer dissociation. And this requires a supply of energy to accomplish it. Warington has drawn attention to the striking fact that in the case of the nitrifying bacterium, assimilation may go on without the intervention of chlorophyll, the energy being supplied by the oxidation of ammonia. This brings us down to the fact, which has long been suspected, that protoplasm is at the bottom of the whole business, and that chlorophyll only plays some subsidiary and indirect part, perhaps, as Adolph Baeyer long ago suggested, of temporarily fixing carbon oxide like hæmoglobin, and so facilitating the dissociation.

Chlorophyll itself is still the subject of the careful study by Dr. Schunck, originally commenced by him some years ago at Kew. This will, I hope, give us eventually an accurate insight into the chemical constitution of this important substance.

The steps in plant metabolism which follow the synthesis of the proto-carbohydrate are still obscure. Brown and Morris have arrived at the unexpected conclusion that "cane-sugar is the first sugar to be synthesised by the assimilatory processes." I made some remarks upon this at the time (*Journ. Chem. Soc.*, 1893, 673), which I may be permitted to reproduce here.

"The point of view arrived at by botanists was briefly stated by Sachs in the case of the sugar-beet, starch in the leaf, glucose in the petiole, cane-sugar in the root. The facts in the sugar-cane seem to be strictly comparable (*Kew Bulletin*, 1891, 35-41). Cane-sugar the botanist looks on, therefore, as a 'reserve material.' We may call 'glucose' the sugar 'currency' of the plant, cane-sugar its 'banking reserve.'

"The immediate result of the diastatic transformation of starch is not glucose, but maltose. But Mr. Horace Brown has shown in his remarkable experiments on feeding barley embryos that, while they can readily convert maltose into cane-sugar, they altogether fail to do this with glucose. We may conclude, therefore, that glucose is, from the point of view of vegetable nutrition, a somewhat inert body. On the other hand, evidence is apparently wanting that maltose plays the part in vegetable metabolism that might be expected of it. Its conversion into glucose may be perhaps accounted for by the constant presence in plant tissues of vegetable acids. But, so far, the change would seem to be positively disadvantageous. Perhaps glucose, in the botanical sense, will prove to have a not very exact chemical connotation.

"That the connection between cane-sugar and starch is intimate is a conclusion to which both the chemical and the botanical evidence seems to point. And on botanical grounds this would seem to be equally true of its connection with cellulose.

"It must be confessed that the conclusion that 'cane-sugar' is the first sugar to be synthesised by the assimilatory processes seems hard to reconcile with its probable high chemical complexity, and with the fact that, botanically, it seems to stand at the end and not at the beginning of the series of metabolic change."

Protoplasmic Chemistry.

The synthesis of proteids is the problem which is second only in importance to that of carbohydrates. Loew's views of this deserve attentive study. Asparagin, as has long been suspected, plays an important part. It has, he says, two sources in the plant. "It may either be formed directly from glucose, ammonia (or nitrates) and sulphates, or it may be a transitory product between protein-decomposition and reconstruction from the fragments" (*loc. cit.*, 64).

In the remarks I made to the Chemical Society I ventured to express my conviction that the chemical processes which took place under the influence of protoplasm were probably of a different kind from those with which the chemist is ordinarily occupied. The plant produces a profusion of substances, apparently with great facility, which the chemist can only build up in the most circuitous way. As Victor Meyer (*Pharm. Journ.*, 1890, 773) has remarked: "In order to isolate an organic substance we are generally confined to the purely accidental properties of crystallisation and volatilisation." In other words, the chemist only deals with bodies of great molecular stability; while it cannot be doubted that those which play a part in the processes of life are the very opposite in every respect. I am convinced that if the chemist is to help in the field of protoplasmic activity he will have to transcend his present limitations, and be prepared to admit that as there may be more than one algebra, there may be more than one chemistry. I am glad to see that a somewhat similar idea has been suggested by other fields of inquiry. Prof. Meldola (Nature, xlii. 250) thinks that the investigation of photochemical processes "may lead to the recognition of a new order of chemical attraction, or of the old chemical attraction in a different degree." I am delighted to see that the ideas which were floating, I confess, in a very nebulous form in my brain are being clothed with greater precision by Loew.

In the paper which I have already quoted, he says of proteids (*loc. cit.*, 13): "They are *exceedingly labile compounds* that can be easily converted into relatively stable ones. A great lability is the indispensable and necessary foundation for the production of the various actions of the living protoplasm, for the mode of motions that move the life-machinery. There is a *source of motion* in the labile position of atoms in molecules, a source that has hitherto not been taken into consideration either by chemists or by physicists."

But I must say no more. The problems to which I might invite attention on an occasion like this are endless. I have not even attempted to do justice to the work that has been accomplished amongst ourselves, full of interest and novelty as it is. But I will venture to say this, that if capacity and earnestness afford an augury of success, the prospects of the future of our Section possess every element of promise.

PHYSICS AT THE BRITISH ASSOCIATION.

The proceedings of this Section were commenced by the delivery of the presidential address by Prof. W. M. Hicks. In seconding the vote of thanks to the President, Prof. Fitzgerald referred to the possible change of mass with temperature, suggested in the address, and pointed out that such a phenomenon would show itself by a deviation of planetary motions from strict conformity to Kepler's laws, owing to their change of mass on cooling.

Sir Douglas Galton exhibited plans of the German Reichsanstalt, and of the new buildings in course of construction, and gave a more detailed account of the management of this institution than is contained in his presidential address to the Association. His object in reading the paper was to revive a movement set on foot at a previous meeting by Prof. Oliver Lodge. The Committee appointed at that time to consider the question of a National Physical Laboratory for the United Kingdom made but little progress, possibly because they did not propose to develop any existing institution. He suggested that the scope of the Kew Observatory should be extended so as to include research, and that it be made the starting-point for the national laboratory.

A discussion followed, in which several members took part. Prof. Rücker lamented the want of concentration and organisation in research work, and thought a national laboratory might remedy this. He regretted that the day was passing away when a man could undertake both teaching and research, because, in his opinion, teachers should not give up research. Prof. Oliver Lodge drew attention to the enormous advantages possessed by a national institution, for carrying on researches extending over a long period. In a university laboratory such research would possibly be discontinued with a change of professor. The universities would still do pioneer work, discovering new fields of research and obtaining preliminary results. Prof. Fitzgerald, on the other hand, did not think it advisable to hand over research to a national laboratory, whereas he strongly advocated an extension of the standardising work performed at Kew. He believed that the highest kind of instruction was training in research work, and it was the function of the universities to give this instruction. Instead of that, the professors are called upon to cram old knowledge into immature and stupid students. The Section has appointed a Committee to reconsider the question of a national laboratory.

Prof. Henrici read a paper on the teaching of geometrical drawing in schools, which was, he said, as a rule very bad. He pointed out that Euclid's constructions are generally followed, the use of the set-square being discarded and only straight-edges and compasses used. He urged the desirability of discarding Euclid in the teaching of geometrical drawing, advocating the use of the set-square from the very commencement. The examples ought to be so arranged that a student can verify his constructions for himself; he therefore suggested the appointment of a Committee to report on the whole question and issue